Duration of Labor and Cesarean Delivery in Association with Epidural Analgesia in Nullipara

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I. SUMMARY

Epidural analgesia is a safe and effective method for pain relief during labor. It is commonly used in obstetric practice and its popularity is ever increasing. However, questions have been raised whether epidural analgesia prolongs labor and increases risk of instrumental and cesarean deliveries. Previous literature on this issue is limited and inconsistent. A randomized clinical trial is extremely difficult to conduct because of the widespread use of epidural analgesia in the U.S.; previous observational studies were often flawed with biases. Well designed studies are warranted.

This historical cohort study will compare the duration of labor and incidence of cesarean and instrumental delivery before and after the availability of epidural use upon request in the Tripler Army Medical Center. Nulliparous women who had singleton gestation in vertex presentation, spontaneous onset of labor at 37-42 weeks, who gave birth from October 1, 1992 to September 30, 1993, and from October 1, 1995 to September 30, 1996 will be selected from computerized obstetric data base. These two groups will be called "Before" and "After" groups, respectively. Detailed information on labor and delivery will be extracted on a standardized form from medical records. Duration of labor, rate of cesarean and instrumental deliveries will be compared between the Before and After groups. In addition, we will directly compare selected women who had epidural analgesia with controls who did not have epidural on the course of labor and incidence of cesarean and instrumental delivery. Results from this study will have important clinical implications in obstetric and anesthesia practice.

II. BACKGROUND AND SIGNIFICANCE

Epidural analgesia is a safe and effective method for pain relief during labor. It is commonly used in obstetric practice and its popularity is ever increasing. However, questions have been raised whether epidural analgesia prolongs labor and increases risk of instrumental delivery and cesarean delivery. Unfortunately, previous literature has generated inconsistent results, which obscures the guidance to obstetricians and anesthesiologists in their practice.

In order to identify critical issues related to this complicated topic, we have systematically reviewed published literature. We conducted a MEDLINE search using key words such as "epidural", "labor", "forceps", "cesarean", "delivery". Papers in English from 1965 to October 1997 were potentially eligible for this review. We narrowed down to papers that focused on the effects of epidural analgesia on duration of labor and mode of deliveries. We collected these articles and searched for other potentially eligible studies by cross-checking all the references in these reports. We identified 30 published original studies that presented useful data. We abstracted key information from each study, such as study design, criteria for eligibility, intervention, local anesthetics used for epidural and nonepidural analgesia, results and potential problems. We assigned a numeric score (0-5, 5 being the best quality) to each study based on the following criteria:

5 Well designed, carefully conducted randomized clinical trials;
4 Randomized clinical trials which was not optimal in certain aspects, such as eligibility of participants being less rigorous in comparison with those scored "5" but the randomization was fully executed; OR excellent observational studies in which no obvious bias could be identified or potential biases were carefully controlled;
3 Observational studies in which the controls were carefully sought out and adjustment for major potential biases was made; however, residual confoundings appeared to be likely;
2 Observational studies in which the controls were selected based on less strict criteria and adjustment for potential biases was poorly done;
1 Observational studies in which the controls were selected based on loose criteria (convenient samples) and no adjustment for biases was done;
0 Observational studies without appropriate controls.

Given the fact that there is a great variation in study quality, we believe only studies that meet minimal requirements deserve further evaluation and analysis. Such restriction will minimize the possibility of including misleading results and hopefully will provide us valid conclusions. Table 1 presents the selected study with a quality score of 3 or above and key information.

A well-conducted randomized trial from Denmark (Philipsen, 1989) compared 57 patients with epidural and 55 patients with pethidine. No statistically significant differences in duration of labor (805 min vs. 657 min.) or cesarean delivery rate (17% vs. 11%) were found. The second trial by Thorp et al.(1993) compared 48 women who were randomly assigned to receive epidural and 45 women narcotic analgesia. The group receiving epidural had a significant prolongation in the first and second stages of labor, an increased requirement for oxytocin augmentation, and a significant increase in cesarean delivery for dystocia. Chestnut et al. also conducted three randomized clinical trials, two of which examined the effects of continuous epidural analgesia on the 2nd stage of labor (Chestnut 1987a, Chestnut 1987b). Although 0.75% lidocaine had no effects, continuous 0.125% bupivacaine was shown to prolong the 2nd stage of labor and increase instrumental delivery but have no effects on cesarean delivery rate. In a later study, the authors found no differences in labor course and mode of delivery between early and late epidural placement of continuous 0.125% bupivacaine, defined as epidural before and after cervical dilation of 5 cm, respectively (Chestnut 1994).

Among the observational studies, four of them consistently showed that epidural analgesia with

bupivacaine increased duration of labor, cesarean and instrumental delivery rates (Liberman 1996; Diro 1985;Thorp 1991; Newton 1995). However, the major concerns over retrospective observational studies was the selection bias, i.e., patients with more abnormal labors will experience more pain and therefore self-select themselves into the epidural groups (Thorp 1994). Thus, longer duration of labor and higher incidence of cesarean rate among epidural users may simply be the consequences of difficult labor. On the other hand, women who do not use epidural, if readily available, are more likely to experience smoother and less painful births. Therefore, direct comparison between these two groups of women in a retrospective study may be invalid. Second, the association between epidural use and instrumental vaginal delivery (forceps and vacuum extraction) is even more complex. It is unclear and difficult to distinguish whether epidural increases forceps use or obstetricians use forceps more liberally in patients with an epidural (Chestnut 1991). It should be noted that all the studies presented here have adjusted for factors that may have major impact on labor course. Among these retrospective studies, the most convincing evidence was illustrated by Lieberman et al.(1996), who evaluated the effect of epidural analgesia on cesarean deliveries in 1733 low-risk, term nulliparas with singleton infants in vertex presentations, in which labor began spontaneously. Using propensity scores to create five subgroups of women who appeared equally likely to receive epidural analgesia, they found women receiving epidural analgesia was 3.7 time more likely to undergo a cesarean (adjusted 95% confidence interval 2.4-5.7). The greatest increase in cesarean risk was noted when epidural analgesia was administered earlier in labor, but there was a more than twofold increase regardless of the dilation and station at administration of epidural analgesia.

There is another category of observational studies, namely ecological study or natural experiment, that is also noteworthy. Instead of comparing women with and without epidural during the same time period, the ecological studies compared the rates of cesarean and instrumental deliveries before and after the epidural analgesia was instituted. Chandler and Davidson (1982) presented first such kind of data from University of Leeds, UK (Figure 1). From 1969 to 1978, the rate of epidural analgesia in labor increased from essentially not available to 37%. However, forceps use remained unchanged. Rate of cesarean delivery increased slightly, from 6.5% to 11%. However, no detailed information regarding changes in the obstetric population and other obstetric practice was presented. The modest increase in cesarean rate across a ten year period was unexplained.

Table 1. Selected studies on epidural analgesia in relation to duration of labor and mode of delivery

Author/year (quality score) reference	Purpose	Study design	Eligibility of patients	Epidural Analgesia regimen	for controls	Results	N	Duration of labor (min.) 1st	2nd	Cesarean delivery overall	Instrumental delivery (%)	(%)
Chestnut, 1987 (5)	Effects of continuous lidocaine in 2nd stage	double blind RCT	nullipara, singleton, term, vertex	continuous 0.75% lidocaine in 2nd stage	placebo in 2nd stage	*Epidural*	26	--	73		0	30
						Nonepidural	27	--	76		4	33
Chestnut, 1987 (5)	Effects of continuous bupivacaine in 2nd stage	double blind RCT	nullipara, singleton, term, vertex	continuous 0.125% bupivacaine in 2nd stage	placebo in 2nd stage	*Epidural*46		--	124*		13	52*
						Nonepidural	46	--	94*		13	27*
Philipsen, 1989 (5)	Effects of epidural on labor course	RCT	singleton, term, vertex	top-up 0.375% bupivacaine in 1st stage	pethidine 75mg i.m.	*Epidural*	57	197	47	805	17	25
						Nonepidural	54	180	37	657	11	26
Thorp, 1993 (5)	Effects of epidural on labor course	RCT	nullipara, singleton, term spontaneous onset, vertex	continuous 0.125% bupivacaine throughout	narcotics	*Epidural*	48	676*	115*		25*	19
						Nonepidural	45	519*	54*		2*	11
Chestnut, 1994 (5)	Effects of early vs late epidural on labor course	RCT	nullipara, singleton, term spontaneous onset, vertex	continuous 0.125% bupivacaine starts at 3-5 cm cervical dilat.	nalbuphine 10mg i.v. till ∃5 cm, 0.125% bupivacaine thereafter	*Epidural*	172	329	85		10	37
						Nonepidural	162	359	88		8	43

*: differences between corresponding groups were statistically significant. $p < 0.05$

Table 1. Continued

Author/year (quality score) reference	Purpose	Study design	Eligibility of patients	Epidural Analgesia regimen	for controls	Results	Duration of labor (min.) N	1st	2nd	overall	Cesarean delivery (%)	Instrumental delivery (%)
Gribble, 1991 (4)	Comparison before vs after epidural became available	retrospective observational	singleton, vertex without previous cesarean section	continuous 0.125% bupivacaine	other analgesics or none	*Before (nullipara only)*	526				17	
						After (nullipara only)	425				16	
Lieberman, 1996 (4)	Effects of epidural on mode of delivery	retrospective cohort spontaneous	nullipara, term, vertex bupivacaine onset, singleton	continuous 0.125% + 2ug/ml fentanyl	other analgesics or none	*Epidural*	991				17*	19*
						Nonepidural	742				4*	4*
Lyon, 1997 (4)	Comparison before vs after epidural became available	retrospective observational	nullipara, singleton, term, vertex	continuous bupivacaine + fentanyl	other analgesics or none	*Before*	373	485	69*	569	12	21
						After	421	483	79*	573	10	25
Diro, 1985 (3)	Effects of epidural on labor course	matched singleton, prospective observational	spontaneous onset without PROM	top-up 0.25% bupivacaine	other analgesics or none	*Epidural*	43	905*	62*	968*	16*	26*
				Matching on race, age, parity, gestational age, birthweight.		*Nonepidural*	43	617*	45*	662	0*	9*
Thorp, 1991 (3)	Effects of epidural on labor course	retrospective observational	nullipara, term singleton, spontaneous onset, cervix # 5cm	continuous 0.125% bupivacaine	other analgesics or none	*Epidural (Early cervical dilation ∃1cm/h, epidural before 5cm)*	45	342*	72*		7	39*
						Nonepidural (Early cervical dilation ∃1cm/h, no epidural)	117	234*	48*		3	9*
Newton, 1995 (3)	Effects of epidural on uterine function	prospective observational	singleton, vertex	continuous 0.125% bupivacaine + 2 ug/ml Fentanyl	meperidine or promethazine	*Epidural*	62	1.6cm/h	40*		5	14*
						Nonepidural	124	3.5cm/h	15*		0	2*

*: differences between corresponding groups were statistically significant. $p < 0.05$

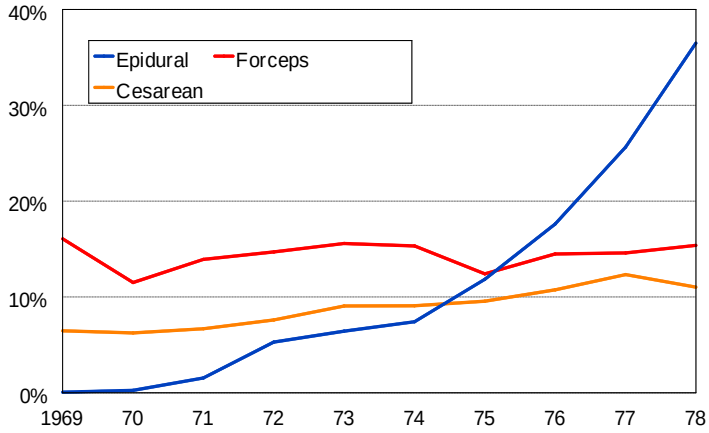

Figure 1. Epidural analgesia at University of Leeds, UK. (0.5% bupivacaine, "top-up" at L_{2-4})

Gribble and Meier (1991) reviewed 1084 hospital records who were delivered during 15 months (5/1989--7/1990) in which there was a 24-hour "on demand" epidural service (epidural rate 48%). This was compared with primary cesarean rate during 15 months (9/1986--11/1987) in which epidural analgesia was not available, even on physician request. These two groups were from the same population based managed by the same eight obstetricians using the same management techniques. The primary cesarean rate was 9.0% before and 8.2% after the epidural service began. No information on labor course was available. More recently, Lyon et al.(1997) presented data from the U.S. Air Force Medical Corps at Andrews Air Force Base, MD. On October 1, 1993, a sudden change in military requirements mandated provision of elective labor epidural capability at that institute. The authors reviewed labor and delivery logs from the year before this date (epidural 13%) and the year after it (epidural 59%). Only term singleton with vertex presentation, nulliparity with anticipated vaginal delivery were included. There were no differences in cesarean or instrumental delivery rates, mean duration of 1st stage of labor between these two periods. However, mean duration of the second stage of labor and number of second stage > 2h increased significantly. The incidence of chorioamnionitis also more than doubled in the later period.

The potential bias in conventional observational studies and the inconsistency between 'natural experiments' and other observational studies point the research direction toward randomized clinical trials. Several groups of clinicians have tried to conduct a randomized trial to test this association. However, given the widespread use of epidural anesthesia in Western countries, it becomes increasingly difficult, if not impossible, to randomize patients to receive either epidural or an alternate (often less effective) analgesic technique. Furthermore, decision on operative delivery is often confounded by subjectivity. It is unfeasible to blind obstetricians to the methods of analgesia (epidural versus nonepidural). Therefore, it is difficult to ensure that the same criteria for cesarean section are used in both randomized groups.

Given the fact that randomized clinical trial seems unfeasible in the U.S., and previous observational studies were limited, inconsistent, and often flawed with methodological deficiencies, a well-designed study is warranted. Answers to this issue will have important clinical implications.

III. HYPOTHESIS

Null hypothesis: Use of epidural analgesia does not prolong labor or increase risk of cesarean delivery.

IV. STUDY POPULATION AND RATIONALE

The prevalence of epidural use in the majority of hospitals has increased gradually, spanning over 10 years. During the same period, most hospitals in the U.S. also experienced rapid changes in the rate of cesarean delivery. Therefore, under most circumstances, historical comparison in cesarean delivery rate before and after the introduction of epidural is invalid. On the other hand, selecting patients within several consecutive years may not solve the issue of different obstetric practice and types of anesthetics for epidural analgesia. Further, selection bias seems inevitable, even if the study population is confined to only one year. Fortunately, there exits a unique opportunity for us to avoid all these critical deficiencies in an observational study.

The Tripler Army Medical Center in Honolulu, Hawaii, is a large tertiary hospital and the only medical facility in the military base, serving the military personnel and their family. There are about 2,800 deliveries per year. The patient population is well defined with mixed race/ethnicity (Asian, Black, Hispanic and White). Universal access to health care and relative homogeneity in socioeconomic status and physical environment provides an excellent condition to study physiological/pathological effects with less subjectivity related to patient service status (private/clinic as in other studies) (Baker 1994). Most importantly, the use of epidural analgesia was very low (6%) before October 1993. The main indication of epidural was severe preeclampsia. However, in October 1993 with instant change in the military Anesthesia Service's policy in which epidural analgesia became available upon request, the prevalence of epidural use jumped to 65% in one year. This dramatic change offers an excellent opportunity to study issues related to epidural analgesia.

V. STUDY DESIGN AND METHODS

1. Overview

This historical cohort study will compare the duration of labor and incidence of cesarean and instrumental delivery before and after the availability of epidural use upon request in the Tripler Army Medical Center. Nulliparous women who had singleton gestation in vertex presentation, spontaneous onset of labor at 37-42 weeks, who gave birth from October 1, 1992 to September 30, 1993, and from October 1, 1995 to September 30, 1996 will be selected from computerized obstetric data base. These two groups will be called "Before" and "After" groups, respectively, hereafter. Detailed information on labor and delivery will be extracted from medical records onto a standardized form. Duration of labor, rate of cesarean and instrumental deliveries will be compared between the Before and After groups. In addition, we will directly compare selected women who had epidural analgesia with controls who did not have epidural on the course of labor and incidence of cesarean and instrumental delivery. Results from this study will have important clinical implications in obstetric and anesthesia practice.

2. Identification of Eligible Subjects

From the computerized obstetric database, women who meet the following criteria and gave births from October 1, 1992 to September 30, 1993 or from October 1, 1995 to September 30,

1996 will be selected:

X nullipara;
X singleton gestation;
X mother's age between 18 and 34 years inclusive;
X gestational age between 37 and 42 completed weeks inclusive;
X birthweight between 2500 g and 4000 g inclusive.

A small database will be created, which only includes those who meet the above criteria. A list of patients' name and hospital record identification number will be printed. Since the obstetric database has limited information, the following additional inclusion criteria will be searched through the medical records manually.

X vertex presentation at admission;
X cervical dilation < 7cm at admission;
X not a precipitate labor (< 3 hours from onset of labor)
X not scheduled for cesarean delivery (i.e., not an elective cesarean);
X no induction of labor (i.e., spontaneous onset of labor);

If the patient meets all the above criteria, the following information will be extracted from the records: (see Appendix I for detail).

Sociodemographic characteristics;
Prenatal care;
Admission assessment;
Course of labor and delivery;
Anesthesia use;
Postpartum complications and neonatal outcomes.

3. Data Collection and Management

A full-time study coordinator will be hired for this project. S/he will be responsible for coordinating different aspects of the project, managing daily tasks, extracting data and being a liaison to investigators at NIH, principal investigator at Tripler and residents and students who extract the data. Since the volume of record retrieval and data extraction is substantial and familiarity of the obstetric and anesthesia chart and, occasionally, clinical judgement are required, it is desirable that residents in obstetrics/gynecology and anesthesiology be involved in the record review. This exercise can also serve as a training experience for residents and students who are potentially interested in research in their future career.

The study coordinator will distribute the study protocol and forms to each person involved. An instruction session will be held with all parties involved, including obstetrician, anesthesiologist, residents, and staff at Medical Record. The principal investigator will briefly introduce the project, explain the procedures in data collection (see below) and go through the data extraction form. Questions raised in the meeting should be clarified by the principal investigator. The residents will then be asked to extract data from two sample records as an exercise. The sample

records will be selected by the principal investigator. All the residents, as well as the principal investigator, will extract data from the same records. The purpose of this exercise is two-folded: to familiarize themselves with the procedure and forms, and to identify problems and misinterpretation. A review session will be held with the principal investigator. Questions and issues found during the exercise should be discussed and resolved. Any necessary change on the forms will be made in consultation with the investigators at NIH. Since people who extract data are aware of the purpose of this project, it should be stressed that they be as objective as possible and not be influenced by their personal opinion when extracting data, especially when information on the medical record is ambiguous.

The study coordinator will work with the computer staff responsible for the obstetric database to generate a list of subjects who meet the first five inclusion criteria. S/he will assign a certain number of records to each resident and student, half of which will be from the 1992-1993 period and half from the 1995-1996 period, to minimize possible investigator bias. If a medical record has incomplete information or is illegible, the person should consult the principal investigator for clarification. If information is missing and cannot be deduced based on informed judgment in consultation with the principal investigator, denote "missing". If the information is ambiguous or extraordinary, take notes on the form or make a photocopy of the record. Completed forms will be returned to the study coordinator. S/he will be responsible for checking the completeness and legibility. Completed formed will be sent to NIH by express mail once a month. Information on completion and shipment of the forms will be recorded on a log book.

As a quality control measure, the principal investigator will randomly select and review 10% of the records and the corresponding forms filled by each resident every month, in order to minimize between-person and within-person inconsistency. This procedure is critical especially at the early stage of the project.

Data will be computerized at NIH. The data entry programs will include range and validity checks for each field. Any data value that fails a range or validity check will alert the computer operator and the program will not proceed until the problem is resolved. The database will be used to export Statistical Analysis System (SAS Institute, Inc. Cary, NC) data sets for statistical analysis. Inspection of the distributions of all exposure and outcome variables will be performed, and outlier or inconsistent results that passed the range and validity checks at data entry will be verified. Any change made to a data collection form or the database will be entered in a log book that includes the subject identification number, original value, corrected value, person authorizing the correction, and date the correction is made.

4. Data Analysis

We will first produce descriptive statistics for each of the outcome variables and key independent variables. For naturally categorical variables, we will tabulate frequencies. For continuous measures, we will produce graphical and statistical summaries of the distributions and consider the need for transformation to obtain more normal distributions. For some variables, such as Apgar score, we will look for natural cutpoints for categorical analyses. Statistical basis for categorization will be reconciled with biological interests to arrive at a tentative choice of cutpoints. If no natural cutpoints can be identified, percentiles will be used.

Historic comparison between "Before" and "After"

In order to make a valid comparison on course of labor and cesarean delivery rate between "Before" and "After" periods, we will first compare demographic characteristics of these two groups (e.g. mean age, race/ethnicity, military status, mean birthweight) and obstetric characteristics at admission (e.g., position, dilation, effacement, length of labor). Only when these baseline variables are similar will the direct comparison on outcome variables be meaningful. The following outcomes will be evaluated: duration of 1st and 2nd stages of labor, oxytocin use for augmentation, incidence of instrumental delivery, cesarean delivery rate, and causes of cesarean delivery. Student t-test and chi-square test will be used for continuous and categorical variables, respectively.

If the "Before" and "After" groups differ in certain baseline characteristics, which could potentially affect labor course and cesarean rate, we will use multivariate analysis to adjust for potential confounders. To assess which variable is a confounder, the following steps will be performed. First, variables that potentially affect labor course or cesarean delivery within our data will be considered as possible confounders. For those variables, we will examine their confounding effect on exposure, comparing "crude" and adjusted odds ratios using logistic regression for cesarean delivery and "crude" and adjusted beta-coefficient using multiple linear regression for duration of labor. Variables that change the "crude" odds ratio or beta-coefficient by 10% or more will be considered as confounders and adjusted in the final analysis. If several such confounders are identified, we will control them simultaneously to obtain a fully adjusted results. Thus, in the final model, the association between exposure ("Before" versus "After" period) and outcomes will be examined adjusting for other potential confounders. The results will indicate the independent effect of exposure.

Results from the above analyses will demonstrate whether increase in epidural analgesia use is associated with prolonged duration of labor and increased rate of cesarean delivery among nulliparous women.

Direct comparison between women with and without epidural analgesia

Although the above results will provide an ecological and historical view, a direct comparison between those who had epidural and those who did not is difficult. This is a more critical issue and the answer has been vigorously sought by many investigators. In order to make such a comparison in an observational study, measures must be taken to eliminate potential selection bias discussed above. We will use women who had epidural analgesia during the "After" period as the exposed group. The major difficulty is to select an appropriate control group who did not use epidural in labor. Women who did not use epidural at the same "After" period are inappropriate controls because of selection bias. Therefore, we have to select women in the "Before" period who did not use epidural but who could have had one if it had been available at that time.

Two methods are worth considering. First is extensive matching, in which women without epidural in the Before group will be matched with women with epidural in the After group on

basic and obstetric characteristics. We assume that because these women had the same characteristics, they would have the same possibility of using epidural analgesia. However, extensive matching requires a very large pool of potential candidates. The sample size in this project (see below) is far from adequate for such method. Therefore, we have to take a slightly less rigorous approach to accomplish the control selection. We will apply propensity score methods (Rosenbaum et al.1984).
Briefly, the propensity score is the probability that a woman would receive epidural analgesia conditional on her baseline characteristics. A stepwise logistic regression analysis will be performed within the "After" group to determine which characteristics remain significant predictors of epidural analgesia use. We will then plot the distribution of propensity scores for women with and without epidural and identify a cut-off point where majority of women with epidural will have a score beyond that point and majority of women without epidural will have a score below that point. Using the above logistic model, we will calculate a score for each woman in the "Before" group quantifying the probability that she would have received epidural analgesia, if it had been available. We will apply the cut-off point to the "Before" group. Subjects who have a propensity score above the cut-off point will be selected as a control group.

Once the appropriate control group is selected, we will compare the exposed (epidural users in "After") with the controls on baseline characteristics. If they are similar, we will compare the course of labor and incidence of instrumental and cesarean delivery between the exposed and controls. Results from these analyses will enable us to directly address whether epidural analgesia prolongs labor and increases risk of instrumental and cesarean delivery.

5. Sample size calculation:

There were approximately 2500 births between October 1992 and September 1993 (Before period) and 2900 births between October 1995 and September 1996 (After period). Approximately 46% were nulliparous, giving us 1150 and 1334 births, respectively, to start with. Assuming 70% of them meet all 10 inclusion criteria (see above), we will have about 800-900 births for each period. Assuming the baseline cesarean section rate was 15% in these groups, with 80% of power and two-sided significance level at 0.05, our study will be able to detect a 6% of increase in cesarean delivery, to 21% in "After" period.

In the further analysis on direct comparison between women with and without epidural, however, the statistical power is reduced substantially. According to a previous study (Lieberman, 1996), if the propensity score is used, the prevalence of epidural use is 60% and a cut-off point with a sensitivity of 70% is chosen, we will have only about 24% (60% x 40%) of the subjects to be included, i.e., 200 births with epidural and 200 births without epidural. With this sample size, we will be able to detect a minimum of 12% increase in cesarean rate (i.e., 15% versus 27%, relative risk = 1.8).

6. Duration of the Project and Schedule

January to February, 1998: Preparation, including training and testing of the procedure and forms;
March to August, 1998: Record review and data extraction;

May to September, 1998: Data entry and cleaning;
October to December, 1998: Data analysis and paper writing for publication.

7. Human Subjects

This study will involve record review only. No human subjects will be contacted. On the data extraction form, we will not record patient's name, address or telephone number. We will assign each subject a unique Subject ID. TAMC will keep a log book which will record patient's hospital chart number with Subject ID. The log book will be kept in a secure place. Only project staff at TAMC will have access to the log book. Investigators and researchers outside TAMC will be blinded to subjects' identity. Information collected in this study will be kept strictly confidential and be used solely for research purpose.

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